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THE SUPPOSED DEATH-RATES OF ABSTAINERS
AND NON-ABSTAINERS AND THEIR
LACK OF SCIENTIFIC VALUE

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attempt to calculate in specific figures the deaths in the United States directly or indirectly due to Alcohol. Price, \$2 per copy, substantially bound in cloth.



The Mortality of Alcohol

A Statistical Approximation of the Deaths
in the United States in Which Alcohol
May Figure as a Causative or
Contributory Factor

By

EDWARD BUNNELL PHELPS, M. A., F. S. S.

Author of "War Risks," "Tropical Hazards,"
"A Statistical Study of Infant Mortality,"
"Nine Years of American Mortality
Statistics," "Neurotic Books and
Newspapers as Factors in
the Mortality of Suicide
and Crime," etc.

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The Mortality of Alcohol

THE use and abuse of alcohol may fairly be regarded as one of the live questions of the day in both Europe and America, and Mr. Phelps's study of "The Mortality of Alcohol" is an important contribution to the literature of the subject. Despite the enormous number of books, papers and addresses dealing with its various phases which have been published in this country, there is no record in type of any serious attempt to reduce to specific figures the probable mortality of alcohol in the United States. No end of "guesses" as to that mortality have been made, and oftentimes vehemently affirmed by partisans on one side or other of the discussion, but their extremes have been as far apart as the poles.

For instance, on September 24, 1911, the Rev. Christian F. Reisner is reported to have made the statement from his pulpit at the Grace Methodist Episcopal Church, in New York City, that "liquor kills 440 out of every 1,000 every year." Assuming the total annual mortality of Continental United States to be about 1,300,000 deaths at this time, this sweeping statement would indicate that the mortality of alcohol in this country amounted to the startling figure of 572,000 deaths a year. In his widely-quoted work on "Alcoholism—A Study in Heredity," Mr. G. Archdall Reid, the well-known English author, places the mortality from the use of alcohol in the United Kingdom at "about one-sixth of the total mortality from all causes"—a figure which, if applied to the total mortality of this country, would suggest that nearly 217,000 deaths per annum in the United States were chargeable to alcohol. These widely-varying "estimates" are fairly indicative of the comprehensive lack of specific information on the subject up to date.

Mr. Phelps's work is the pioneer effort to calculate by scientific methods the approximate mortality of the United States in which alcohol "directly, indirectly, or even remotely figures," and places that mortality at about 66,000, or about 5 per cent. of the total mortality from all causes at all ages, as contrasted

with the Rev. Mr. Reisner's "estimate" of 44 per cent. and Mr. Reid's "estimate" of nearly 17 per cent. for the United Kingdom. The specific figure of about 66,000 deaths arrived at by Mr. Phelps rests on the solid basis of *averages* of the percentage estimates of the distinguished Medical Directors of three American insurance companies, namely, Dr. Brandreth Symonds, of the Mutual Life Insurance Company of New York; Dr. Eugene L. Fisk, of the Postal Life Insurance Company; and Dr. William L. Gahagan, of the United States Casualty Company, all of whom have long given especial attention to the relation of alcohol and insurance risks. These authorities having independently named the percentages of male deaths at adult ages in each of 106 causes of death in which alcohol directly or indirectly figures in their respective opinions, on the *average* of these estimates the number of male deaths in which alcohol presumably figures is computed, and with this figure as a basis the probable alcoholic mortality of the entire country is worked out by a series of carefully-explained statistical processes. Every step of the calculation is open to the sharpest analysis, and at least a starting-point for serious, scientific, discussion of the far-reaching subject has been established.

For twenty years Mr. Phelps has edited one of the leading insurance magazines of this country, "The American Underwriter," has compiled and published many statistical works on insurance and other topics, and is a Fellow of the Royal Statistical Society of London, and a member of the American Statistical Association and many other scientific societies of this country. At the invitation of the President of Section IX—Demography, of the Fifteenth International Congress on Hygiene and Demography, held at Washington, D. C., in September, 1912, Mr. Phelps prepared and presented at the Joint Session of Sections IV and IX (Hygiene of Occupations, and Demography) a paper on "The Mortality from Alcohol in the United States—The Results of a Recent Investigation of the Contributory Relation of Alcohol with Each of the Assigned Causes of Adult Mortality." This paper and the accompanying tables are published in the voluminous Transactions of the Congress, and the pioneer work on "The Mortality of Alcohol"

contains many hitherto-unpublished tabulations of permanent value for reference purposes, thus making the book an indispensable accession to the bibliography of alcohol—and one of especial service to physicians, sociologists, ministers, and other writers and speakers on, and students of, the drink problem. Copies of the work, measuring about $9\frac{1}{2} \times 6\frac{1}{2}$ inches and substantially bound in cloth, will be promptly forwarded “on approval” to public libraries, or may be obtained by remittance of the price, \$2, to the

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PRESS COMMENTS

Mr. Phelps's qualifications for statistical investigation will not be questioned by those familiar with his work as editor of “The American Underwriter,” and as author of a number of books and pamphlets of a statistical nature.—*New York Times*.

The editor of one of the leading insurance magazines of the country, Edward Bunnell Phelps, has provided the first statement based on scientific knowledge of the mortality in which alcohol directly or indirectly or even remotely figures. The book contains many tabulations of statistics.—*The Congregationalist* (Boston).

Mr. Phelps made the pioneer attempt to calculate in specific figures the deaths in the United States directly or indirectly due to alcohol. His work has attracted very general attention in the domain of social economics, and has given him the highest reputation as an investigator and statistician.—*The Weekly Underwriter* (New York City).

This monograph represents the first serious attempt to determine accurately the number of deaths for which alcohol is responsible annually in this country. Mr. Phelps is to be commended for having adopted so

reasonable and systematic a plan as a basis for his estimates. The figures reached appear to be far more trustworthy than any "guesses" hitherto advanced.—*New York Medical Journal.*

The author is well qualified for such work, inasmuch as for over twenty years he has edited one of the leading insurance magazines in this country, and in other ways has maintained his interest in the cause of death. Taking into consideration the difficulties which enter into such calculations, it is probably a just estimate. The work is well done, and is an interesting contribution.—*The American Journal of the Medical Sciences* (Philadelphia).

Quite recently Edward Bunnell Phelps, an insurance expert in mortality statistics, has put into book form the conclusions he draws from an exhaustive study of vital statistics. The figures compiled by Mr. Phelps and the deductions he makes from them will be of service to insurance authorities who are trying to get at the facts, and also to those who are interested in the sociological side of the subject.—Leading editorial in the *New York Commercial*.

This is a statistical study in which the question is discussed from the standpoint of the statistician and actuary rather than from the standpoint of the physician. While under present conditions such estimates can at the best be only relatively correct, they are of the utmost interest and illustrate the necessity and importance of securing adequate registration of the causes of deaths for the entire country as soon as possible.—*Journal of the American Medical Association* (Chicago).

Beside this forceful exposure of the waste of life through one preventable disease, one may well place the new study of "The Mortality of Alcohol," by Mr. Edward Bunnell Phelps, editor of *The American Underwriter* and a well-known statistician. This first effort in the United States to ascertain by statistical methods the probable deaths for which alcohol is wholly or partially responsible bears every evidence of careful, exact work within the limits laid down for the investigation, and affords a basis for further fruitful study of the question. The estimate represents the average of estimates made by medical directors of three large insurance companies based upon not only their insurance experience but upon hospital service and general practice.—*The Scientific Temperance Journal* (Boston).

This approximation is based on a careful and critical analysis of the mortality figures in this country for the year 1908, correlated with the expert opinions of three physicians chosen as medical directors of large American insurance companies. After reviewing the three similar investigations already made on the same subject in England, the author describes his methods of research and details the tabulated data on which his estimates are based. The methods of his investigation seem rational and its temper is entirely dispassionate, temperate and unbiased. His work is another example of the aid which life insurance may render to scientific medicine in the collection and interpretation of data, and to preventive medicine in the presentation and popular diffusion of knowledge about the causes of death and about faulty and hygienic modes of living.—*The Boston Medical and Surgical Journal.*

There is no question that for men interested in insurance matters the problem of the "mortality of alcohol" is one of foremost rank, and it is to their credit that one of the best scientific studies of the matter that has yet appeared comes from one of their number. Mr. Phelps, who has published several statistical studies in similar fields, has taken the total adult mortality in the United States for the year 1908 as the groundwork of his study. His method was simple—he obtained from the medical directors of three large insurance companies their estimate of the percentage of male deaths between the ages of twenty and seventy-four, directly or indirectly due to alcohol, for each of the 106 causes of death listed in the Census statistics for that year. In defense of this method of study it may be granted that medical directors of insurance companies have much broader knowledge of the general subject of mortality and the effect upon it of certain specific causes than even the busiest and most prominent of practitioners of medicine. Moreover, the differences in the individual estimates were such as to lead to the reasonable conclusion that a poll of, say, several hundred physicians would not have resulted in a nearer *average* approximation than the average obtained from the poll of these three physicians only. Without going into minute analysis of the methods used by Mr. Phelps, we may state that to a disinterested mind his use of the statistical tables seems to be quite correct from the standpoint both of statistical science and of every-day methods of arriving at approximations. This, then, forms the first scientifically constructed approximation of the effect of alcohol upon mortality in this country. Mr. Phelps's study gives the firm foundation by which any future results of such activities may be studied and measured.—*The New York Medical Record.*

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BY

EDWARD BUNNELL PHELPS, M.A., F.S.S.

Editor, THE AMERICAN UNDERWRITER, of New York City, and Author of "Tropical Hazards," "American Mortality Statistics for the Nine Years, 1900-1908," "Neurotic Books and Newspapers as Factors in the Mortality of Suicide and Crime," "The Mortality of Alcohol," "The Mortality from Alcohol in the United States," etc.

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THE SUPPOSED DEATH-RATES OF ABSTAINERS AND NON-ABSTAINERS AND THEIR LACK OF SCIENTIFIC VALUE

After a study of all available data on the subject, about two years ago I compiled and published under the title of "The Mortality of Alcohol" a tentative statistical approximation of the adult mortality of the United States in which alcohol possibly might figure as a causative or contributory factor. My findings were based upon the independent percentage estimates for each of 106 causes of deaths kindly made for me by the Medical Directors of three well-known American insurance companies. The book by no means purported to have solved the complex problem with which it dealt but was offered merely as a possible starting-point for detailed scientific discussion of the actual relations of alcohol and adult mortality in this country. It was so accepted by the leading medical journals, and was generally received by the reviewers as a pioneer effort in its field. Since the publication of this preliminary study—or first aid to the injured, very much injured, *facts* in the case—I have endeavored to keep track of the contemporaneous literature of the subject, and to collect all serious contributions to the discussion of the relations of alcohol and human mortality.

The more I have collected, and read and thought on the subject, the more I have been impressed with the widespread

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circulation of misleading figures, and conclusions as to the alleged death-rates of users and non-users of alcohol which in my judgment seem to be unwarranted. The reading of the text of a discussion of the subject before the Association of Life Insurance Medical Directors of America, in the recently-published transactions of that important body for the years 1906-1911, has shown that at least some competent observers concur with my views of the unreliability of many of the figures and conclusions on these lines. And it has therefore seemed worth while, in the interest of a sober discussion of this phase of modern vital statistics, to endeavor to present herewith and calmly weigh the figures and supposed facts which of late years have been repeatedly cited as bearing on the alleged difference between the death-rates of abstainers and non-abstainers.

That there is a difference between the death-rates of the two sections of the community loosely grouped as "drinkers" and "non-drinkers" is extremely probable—in fact, practically certain. A similar statement might safely be made of fat men and lean men, tall men and short men, ministers and lawyers, physicians and bookkeepers, stone masons and cabinet-makers, and almost innumerable other distinctive groups of men. As to the truth of this sweeping generalization, there is not the slightest doubt; confirmations strong as proofs of holy writ are to be found in the great mass of tabulated classifications of life insurance experience for the last fifty years and more, and the carefully-kept vital statistics of England, Germany and some other countries for long stretches of years.

But—and in the case in point this short word is one of momentous importance—how can there possibly be a scientific determination of the difference between the death-rates of any two groups of men until there has been an agreement as to the precise boundaries of the groups? In other words, how can the difference between the death-rates of users and non-users of alcohol be even approximately calculated until there has been a separation of the sheep and the goats, so to speak, that is, the non-drinkers and the drinkers in the community under observation? Until there has been a meeting of minds on definitions of these groups it is no more possible to fix the death-rates of the two groups and the difference between

them than it is to engage in rational argument without a prior agreement on certain premises. This prerequisite for scientific computation of the relative death-rates of the two groups above named apparently has not occurred to the vast majority of the people who have rushed into the discussion of the subject in books, and periodicals, and pulpits, and in the avalanche of Prohibition literature which has swept over this country of recent years.

NOT THE SOUNDEST OF BASES, BUT A QUICKSAND

Aside from the ultra enthusiastic anti-alcoholics, whose emotional embrace of the subject precludes any serious consideration for mere facts and figures, the bulk of the participants in the discussion have plainly been inclined to regard as the soundest of bases for their argument and conclusions the alleged life insurance experience with so-called total abstainers and non-abstainers. On the strength of twenty years' somewhat intimate acquaintance with the statistics of the insurance business, I can only regard this supposed base as a mere quicksand in so far as trustworthy evidence of the actual difference between the death-rates of users and non-users of alcohol is concerned. There is quite an array of life insurance figures which conclusively prove a difference between the death-rates of persons insured in the abstaining and non-abstaining classes, but after a thorough study of all available data on these lines I am satisfied that no competent and unbiased person who has carefully examined these figures will conclude that they by any means prove either one of the two points so vehemently advanced by the Prohibition spokesmen, namely, (1) that they establish the actual difference between the death-rates of drinkers and non-drinkers, and (2) that they afford a mathematical measure of the mortality directly chargeable to the use of alcohol. Of course I do not for a moment expect that I can convince my Prohibitionist friends of either of these negative deductions. It is no easy matter to prove any negative, and to establish to the satisfaction of the average Prohibitionist the unsoundness of any one of his pet arguments is hopeless. I am, however, quite ready to rest with any unbiased reader, or competent student of statistics, the soundness of my deductions from the evidence below presented.

At the Twenty-second Annual Meeting of the Association of Life Insurance Medical Directors of America, held at New York City in October, 1911, Dr. T. F. McMahon, medical director of the Manufacturers' Life Insurance Company, of Toronto, presented a paper on "The Use of Alcohol and the Life Insurance Risk," and included in his paper certain figures showing the mortality experience of some English life insurance companies with separate classes of so-called abstaining and non-abstaining policyholders, which are discussed on subsequent pages of this paper. Dr. McMahon's address was followed by a general discussion of the subject by members of the Association of Medical Directors, and in part the report of the discussion reads as follows:

Dr. Dwight (Medical Director of the New England Mutual Life)—
I have made out some figures in the experience of our Company which I think may be worth while calling to your attention. I am sure that we have all been interested in the figures that have been given, and I am sure that there can be no question as to the accuracy of such figures, but *I am equally sure that if such figures are published without some qualification, they are apt to give a false impression, and they are apt to be accepted by certain portions of the community at an unfair valuation.* I am sure that most of you gentlemen appreciate the importance of having your classes homogeneous, and this particular investigation shows, I think, the value. *There are many more factors, I think, than the one question as to whether an individual drinks or not, which should be taken into consideration. I imagine the mortality of the total abstainer in the Presbyterian clergyman class and the total abstainer in the bartender class would be quite different, and included in these large groups are a great many cases which are influenced by habitat, other habits, occupation, residence, type of life, and many other factors.*

I am not going to take up much of your time, but I have before me figures on habits in our Company for the past sixty years, both on liquor and tobacco. I bring them up to show two or three rather interesting points. In the first place our figures on the habits as to the use of alcohol agree almost exactly with those which have been given us. We divide them into four classes. No man acknowledges, or very few men acknowledge, that they are excessive in the use of liquor. We divide them into the Total Abstainer, Rarely Use, Temperate, and Moderate. By "rarely use" we mean the man who says that he perhaps twice a year at a dinner drinks two glasses of champagne. While we have every reason to believe that this is an honest statement, it is not quite fair to say he is a total abstainer. Otherwise they are divided by the individual's own statement—in many instances in-

correct. In many instances the total abstainer is a total abstainer because he has to be and has been advised to be, but take them as they run, we find the total abstainer with a mortality of 59% of the American Table—these are rough figures; Rarely Use, 71%; Temperate, 84%; and Moderate, 125%. If we let these figures stand as they are, it is accumulative evidence to demonstrate that we ought all to stop drinking the next minute, and that we ought to divide our applicants into abstainers and non-abstainers; but let us see what the effect of tobacco is. We find that the total abstainer from tobacco has a mortality of 57% as against 59% when he is a total abstainer from alcohol; that the Rarely Use is 72% as against 71% from alcohol; that the Temperate is 84% as against 84% for alcohol; and that the Moderate is 93% as against 125% for alcohol. In other words, the mortality on the total abstainer from alcohol is almost absolutely the same as that on the total abstainer from tobacco. It may be said that a man is usually a total abstainer from both, but he is not necessarily so. It does mean, I think, that we are describing the same kind of groups, the same type of man, the same conservative type of man in the total abstainers from both kinds. Then to show you the numbers—we find that out of 180,000 cards, 42,700 were total abstainers from alcohol—at least they said they were—while the total abstainers from tobacco were 41,100—almost exactly the same number, 13,000 rarely used tobacco, while 20,000 rarely used alcohol. *I only bring them up as they are, simply as a suggestion that before we accept all total abstainers on a different premium or before we talk very much about doing it, we had better be sure that it is all due to total abstinence from alcohol or tobacco, and we had better go a good deal further in our investigations in our attempt to make these groups homogeneous, before we here as an Association or as individuals advocate very strongly the wide separation between total abstainers and those who rarely use.*

Dr. Emery (Manhattan Life Insurance Company)—I would like to make a suggestion for statistics in the future, and that is to find out how much a man eats, for oftentimes it is much more important. A man who is an overeater is not likely to live as long as some excessive drinkers. In other words, a man's habits of eating, and whether he performs the daily functions of life regularly and properly, is what we should know, and if we can only know that, I think we would do a great deal more than we do now, and do more for our Companies. It is most important. The man who is known as a club-man, a free liver, is generally an early dier (pp. 473-6, Abstract of the Proceedings of the Association of Life Insurance Medical Directors of America, from the Seventeenth to and including the Twenty-second Annual Meeting, New York, 1912).

Dr. Dwight's observations clearly stated the danger of taking too seriously the figures of the handful of English life com-

panies which maintain separate classes for abstaining and non-abstaining policyholders, and succinctly named some of the many all-important factors of longevity which are generally overlooked in considering experience with these classes. Coming as they do from so professionally conservative an observer as the Medical Director of one of the oldest life insurance companies in this country, they are deserving of most thoughtful consideration on the part of every man who reads them and is desirous of getting as nearly as possible to the real truth of the much-discussed question of the relative death-rates of users and non-users of alcohol. For these reasons I have reproduced them in their entirety as reported in the Proceedings of the Association, and have italicized some of the more striking passages.

A CLASSIFICATION RESTING ON BIASED STATEMENTS OF APPLICANTS

In so far as the remarks relate to the general question, they would seem to rest on a bed-rock basis, and to be entirely without the range of dissent or criticism. But in so far as they recite the supposed experience of the physician's own life insurance company, they are manifestly open to the criticism which in my judgment fairly applies to the returns of all life insurance companies which have made any attempt to classify their policyholders on the basis of their individual drinking or non-drinking habits. On just what grounds does that classification practically rest? In at least the vast majority of cases, merely on the prejudiced statement made by the policyholder to the medical examiner when he was applying for his life insurance in the company which afterward accepted him. To be sure, of late years, and especially in the case of applications for large policies, many, if not most, of the more important companies send inspectors out to secure supplemental information regarding applicants for policies—and in some cases may have investigations of a policyholder's standing and habits made one, two, or three years after the policy was written. But, in the main, practically all of the so-called statistics of life insurance experience as to allegedly abstaining and non-abstaining policyholders solely rest on the statements made by the policyholders at the time of applying for their insurance.

Of what real value in any scientific investigation of the mortality rates of these two groups of policyholders is evidence so gathered? To all intents and purposes, every applicant for life insurance knows at least enough of the attitude of life insurance companies to feel that he is on the defensive, so to speak, when he is undergoing his medical examination, and to be cautious in his answers to any of the examiner's questions as to his habits which might cause his rejection by the company to which he has applied. In short, the applicant is necessarily a biased witness, and even if he does intend to tell the whole truth about his habits, previous sicknesses, etc., he is apt to take a decidedly optimistic view of any possible defects in his qualifications as a first-class life insurance risk.

When the questions as to drinking habits are reached in the examination, ninety-nine per cent. of the applicants who admit drinking at all are reasonably certain to classify themselves as "moderates," or words to that effect, and if asked for details as to the number of drinks which they average for each day are absolutely certain not to *overstate* their daily quantum. In the vast majority of cases, they undoubtedly *understate* the daily average, either deliberately or unconsciously. In view of these well-known facts, how much importance is to be attached to the classification by Dr. Dwight's company, or any other company having only similar evidence—or lack of it—of its army of policyholders in such groups as "rarely use," "temperate," and "moderate"? Probably no two men's honest notions of the precise meanings of these terms in so far as drinking is concerned would exactly tally; daily rations of alcohol which one man might honestly regard as extremely "temperate" undoubtedly would seem decidedly excessive to another type of man, and so on up and down the line.

From my point of view a rating on any such basis of policyholders as to their individual drinking habits would be practically valueless, even though they were to be polled on this question once a year during the entire duration of their policies. As a matter of fact, they are actually polled but once, at the time their policies are taken out, and generally in their early years, and of course their habits may radically change for better or for worse before their deaths are entered up in the mor-

tality of the "total abstainer," "rarely use," "temperate," or "moderate" classes to which they were assigned many years before, and in which they have continuously since been rated. And the same drawbacks on reliable records operate in the case of the policyholder's habits respecting the use of tobacco.

Furthermore, a moment's consideration of the figures stated by Dr. Dwight as showing his Company's supposed experience with these several classes will suffice to raise the most serious question as their probable value. For instance, the total abstaining class are credited with only 59 per cent. of the expected mortality according to the American Experience Table of Mortality, whereas, the "rarely use" class, whom Dr. Dwight defines as "the man who says he perhaps twice a year at a dinner drinks two glasses of champagne," is charged with a mortality of 71 per cent. of the expected, or an increase of 20.3 per cent. over the mortality of the class which does not drink at all. Would even the most enthusiastic Prohibitionist seriously think of contending that two glasses of champagne twice a year would make the user almost one-quarter again as bad a life insurance risk as was the total abstainer, other things being even? I think not. To go a step farther in the demonstration of the absurdity of figures so calculated, note that according to Dr. Dwight's returns the mortality of even the "moderate" users of alcohol accepted by his Company was apparently 125 per cent. of the expected mortality among acceptable life insurance risks, or, in other words, 111.9 per cent. in excess of the mortality of the total abstainers, and 76.1 per cent. in excess of that of the men who drank only two glasses of champagne twice a year at dinners. I venture to believe that no further riddling of these supposed returns for non-drinkers and moderate drinkers is necessary to show that they are not entitled to serious consideration.

The grave question as to the reliability of life insurance returns as means of determining the relative death-rates of abstainers and non-abstainers from alcoholic beverages goes much deeper than this. The classification of policyholders as to their respective drinking habits, when based on their own biased statements while trying to qualify as acceptable life insurance risks—of course, as a rule, many years before their

deaths figure in the classification of the companies' mortality—at best would be an exceedingly dubious proceeding. But that is by no means the weakest link in the chain of alleged evidence as to the relative death-rates of drinkers and non-drinkers. What conceivable warrant is there for rating in one group *all* applicants for life insurance who cannot truthfully and categorically assert that they are total abstainers? By any such extremely loose classification the man who actually drinks may be two or three glasses of wine or beer in the course of an entire year, the man who takes a cocktail or so practically every day in the year, and the man who regularly takes two or three drinks of whisky or other distilled liquor each day are all counted in the single group of non-abstainers. Almost countless other sub-groupings on the basis of what they drink, how much they drink, and how drinking affects them individually, might doubtless be made in the large body of men comprehensively branded as drinking men, or non-abstainers, by the few life insurance companies which undertake so to classify their policy-holders. And by adding up all the deaths in this group, in all probability including more or less men who are comparatively heavy drinkers, as well as the men at the other extreme who practically if not literally are total abstainers, an alleged death-rate for non-abstainers is obtained!

Can any such process be regarded as rational, let alone scientific? The fact that precisely that sort of process has been followed in certain quarters for many years proves nothing, for many foolish things have become habitual. Would it not be equally fair to classify policyholders by their answers to such absurd questions as, for instance, "do you drink coffee," "do you eat Welsh rarebits," "do you take supper after going to the theatre?" Some of the men who had to plead guilty to these implied charges might confine their indulgence in coffee to the demi-tasse after the banquet which they semi-occasionally attended, might not care for Welsh rarebits and not eat more than one a year, or might similarly deviate from the straight and narrow path of gastronomic proprieties by going to supper after the theatre once or twice a year, whereas others who had answered "yes" might be habitually addicted to these hazards of indigestion and belated sleep. Would any sane man

consider it fair to group them all in together, and attempt to fix a conglomerate, quasi-composite, death-rate for the coffee-drinkers, rarebit-eaters, and late-supper contingent on this basis?

I can see no more sense in pretending to work out the death-rate of non-abstainers on a similar basis, *i. e.*, their inability if strictly truthful to set themselves up as total abstainers. In brief, the non-abstainers so classified are about as heterogeneous a body of men, and about as far from a homogeneous body of men, as could well be conceived. And surely there can be no difference of opinion as to the fundamental necessity of at least reasonably homogeneous groups if any classification of the slightest scientific value is to be made. The real issue is by no means the accuracy of mortality statistics thus compiled, but is, it seems to me, what percentage of the apparent excess of mortality in the case of non-abstaining as compared with abstaining policyholders is properly chargeable to their use of alcohol, what percentage to their other habits, what percentage to their occupations, and so on and so on almost *ad infinitum*? There is the real question, as I see it.

MANY FACTORS CONTRIBUTE TO FIX CLASS MORTALITY

To illustrate the difficulties of credibly and scientifically grouping the death-rates of various classes, suppose we look at the thoroughly reliable mortality statistics of England and Wales, admirably classified by occupations for many years by the Registrar-General's office in its decennial supplements. By the latest of these supplements issued in 1908 we find that the mortality of clergymen, priests and ministers at ages 35-45 in England and Wales in 1900-1902 was 4.06 per 1,000, whereas that of general shopkeepers at the same ages was 19.71 per 1,000, or nearly five times as high. Would it be safe to assume on the strength of these figures that the shopkeeper's occupation was nearly five times as hazardous as that of the clergy? Dozens of similarly striking contrasts might be cited, but perhaps no further evidence is needed to make it clear that almost countless factors contribute to fix the mortality rate of any particular group of lives, and that it would be absurd to attempt to explain the difference between the mortality rates of any two groups by citing any *single* factor present in one case and supposedly absent in the

case of the other group. That is precisely what is done by the vast majority of people who attempt to prove by the comparatively limited experience of the few English companies with separate classes of abstainers and non-abstainers the supposed difference in the probable longevity of people who do and people who do not use alcohol.

In the discussion before the Association of Medical Directors, as shown by the extracts on previous pages of this paper, Dr. Emery said: "I would like to make a suggestion for statistics in the future, and that is to find out how much a man eats, for oftentimes it is much more important. A man who is an over-eater is not likely to live as long as some excessive drinkers." Have any of the discussions of the apparent death-rates of abstaining and non-abstaining groups in English life companies maintaining such groups of policyholders seriously taken into account the question as to the eating habits of the two groups? Not in so far as I can recollect. Nevertheless, the bad physiological effects of over-eating in the way of undue strain on the digestive organs and the clogging of the whole system, and the resultant tendency toward arterio-sclerosis, apoplexy, and other degenerative diseases are so well known as to call for no enumeration here. I have had many eminent physicians tell me they had no question that more people were killed by over-eating than by over-drinking. But practically no consideration seems to be paid to this important factor by the people who triumphantly cite the apparent excess of the death-rate of non-abstainers over that of total abstainers.

While it is scarcely safe to trust to generalizations as to the types and general habits of men who do and men who do not drink, it possibly might be guardedly said that as a rule that section of the modern community which would commonly be regarded as the drinking class leads more active and strenuous lives, keeps later hours, and in both their work and play is subject to more strain than are those who never indulge in any form of alcohol as a means of relaxation. Perhaps it would not be going too far to say that, all things considered, the total abstainers and the non-abstainers are two entirely different types of men—although of course there are many notable exceptions. Assuming for the moment that this hypothetical type-differen-

tiation is correct, might it not be expected that there would be more or less difference in the death-rates of the two types of men, even though alcohol were left out of the argument? I think there can be little question on that score. If such is the case, it is manifestly a case of unsound reasoning and unscientific conclusions to set up the difference between the death-rates of abstaining and non-abstaining groups of life insurance policyholders as an accurate measure of the effect of alcohol on human longevity. That is constantly being done, however, in the impassioned arguments of all the would-be reformers who do not themselves approve of the use of alcohol, and are determined to bring the rest of the world around to their way of thinking.

For the reasons already stated, I am personally unable to regard the oft-quoted figures of the handful of small English life insurance companies which have long maintained separate classes of abstaining and non-abstaining policyholders as evidence of any scientific value as to either (1) the respective death-rates of the drinking and non-drinking classes of the community in general or (2) an accurate measure of the supposed excess of the death-rate of drinking people over that of non-drinking people. If the reasons for this distrust previously cited do not raise a similar question in the minds of all unbiased readers, suppose the figures for the two sections of the oldest and largest of all the English life insurance companies in the field of discussion, the United Kingdom Provident Institution, are brought into the argument and critically analyzed. In his paper presented before the Association of Life Insurance Medical Directors of America, on the subject of "The Use of Alcohol and the Life Insurance Risk," Dr. McMahon included a tabulation of the "mortality experience under ordinary whole life policies" of the "temperance section" and "general section" of the above-named English company for the forty-five years, 1866-1910, arranged by five-year periods. In the figures thus presented were included the *amounts* of expected and actual claims in both sections for each period, but as the *amounts* of claims have absolutely no bearing on the *number* of claims, or deaths, and as the averages of claims in the two sections for the forty-five years were substantially identical—respectively \$1,186 and \$1,167 for the Temperance Section and

the General Section—there would seem to be no reason for clouding the issue with these irrelevant data. Thus edited, the tabulation of the mortality experience of the United Kingdom Provident Institution presented by Dr. McMahon was as follows:

Years	TEMPERANCE SECTION		GENERAL SECTION		RATIOS OF ACTUAL CLAIMS TO EXPECTED CLAIMS	
	Expected Claims	Actual Claims	Expected Claims	Actual Claims	Temperance Section	General Section
1866-70	549	411	1,008	944	74.86%	93.65%
1871-75	723	511	1,266	1,330	70.68	102.60
1876-80	933	651	1,485	1,480	69.77	99.73
1881-85	1,179	835	1,670	1,530	70.82	91.61
1886-90	1,472	1,105	1,846	1,750	68.95	94.79
1891-95	1,686	1,203	1,958	1,953	71.35	99.74
1896-00	1,900	1,402	2,058	1,863	73.79	90.52
1901-05	2,021	1,456	2,221	1,961	72.04	88.29
1906-10	2,291	1,504	2,282	1,900	65.21	83.26
Total, 45 years	12,754	*8,988	15,794	14,711	70.47%	93.14%

At first glance these figures would seem to show a general uniformity, and both demonstrate and sharply accentuate the difference between the death-rates of abstainers and non-abstainers by an experience of forty-five years with a total of 23,699 deaths, or an average of about 527 deaths a year or approximately 10 deaths a week. A study of the figures in detail, however, reveals some puzzling discrepancies. All of the persons whose deaths are included in the above-tabulated mortality presumably had passed the customary medical examination and had been accepted as good life insurance risks. If the only material difference between the two classes was the fact that one class did not, and the other class did, use alcohol, might it not naturally be expected that the difference between the death-rates of the two classes would be a fairly constant differ-

* This total as printed in the Proceedings of the Association differs by 90 from the sum of the figures in the column, indicating a probable typographical error in the column, as the ratio of actual to expected claims, 70.47%, tallies with the total given.

ence, at least if computed by five-year periods and any slight annual discrepancies thus eliminated? If the relative mortality of large groups of policyholders were tabulated for five-year periods by either occupations or localities, a substantially constant difference would be expected unless changes in conditions or other abnormal factors had been interpolated in one or both groups. The "law of average" would so demand, and proof of the fact is to be had by a study of the mortality statistics of England or any other country which has tabulated its deaths by occupations or other sharply-defined groupings for long series of years. On examination of the mortality experience of the two sections of the English life company above presented it will be found that the difference between the death-rates of the abstainers and non-abstainers was by no means constant, except in that it constantly and materially varied, as is shown by the following tabulation of the ratios of actual to expected mortality in the two sections and the excess and percentage of excess of the General Section's ratios, by five-year periods:

Years	RATIOS OF ACTUAL CLAIMS TO EXPECTED CLAIMS		EXCESS AND PERCENTAGE OF EXCESS OF GENERAL SECTION'S RATIOS OVER TEMPERANCE SECTION'S RATIOS OF ACTUAL TO EXPECTED MORTALITY	
	General Section	Temperance Section	Excess	Percentage of Excess
1866-70	93.65%	74.86%	18.79	25.1%
1871-75	102.60	70.68	31.92	45.2
1876-80	99.73	69.77	29.96	42.9
1881-85	91.61	70.82	20.79	29.4
1886-90	94.79	68.95	25.84	37.5
1891-95	99.74	71.35	28.39	39.8
1896-00	90.52	73.79	16.73	22.7
1901-05	88.29	72.04	16.25	22.6
1906-10	83.26	65.21	18.05	27.7
Averages	93.14%	70.47%	22.67	32.2%

This tabulation reveals some surprising variations in the five-year differences between the two sections' ratios of actual to expected mortality, the percentage of the excess of the General Section's ratio over the Temperance Section's ratio of actual to expected having jumped from 25.1 in 1866-70 to 45.2 per cent.

in 1871-75, or nearly doubled. It dropped from 42.9 per cent. in 1876-80 to 29.4 per cent. in 1881-85, or nearly one-third, and in the last three five-year periods was approximately only one-half as large as in 1871-75, and continuously much lower for the fifteen years than in the five-year period immediately preceding that epoch. Why this pronounced shifting of the difference between the ratios of the two sections, and the marked decrease in the last fifteen years, if even the moderate use of alcohol to which non-abstaining accepted life insurance risks are presumably restricted ordains a decidedly increased death-rate for that class as compared with total abstainers? If the body of surviving non-abstaining policyholders were to increase their imbibitions as they grew older, might it not be confidently expected that the excess of their death-rate over that of the total abstainers would show an increase—especially as the total abstainers could not very well lower their own death-rate by drinking less than *nil*? As the above tabulation shows, for the last fifteen years the difference between the death-rates of the two sections has markedly decreased, instead of increased.

NO PRACTICABLE MEASURE FOR A GENERAL DEATH-RATE FOR NON-ABSTAINERS FROM ALCOHOL

In a word, this last tabulation clouds in mystery the real significance of the oft-cited and allegedly conclusive figures of the United Kingdom Provident Institution's mortality experience with its abstaining and non-abstaining classes of policyholders. I can conceive of but one of two explanations of the surprising variations in the difference between the death-rates of the two sections, namely: either the group of non-abstaining policyholders in the General Section is not a sufficiently homogeneous body to promise a constant difference between its death-rate and that of the group of total abstainers—which in at least one way certainly is a homogeneous body—or else the number of lives thus grouped is too small to warrant safe averages. Whichever of these possible explanations is accepted, it would seem that the 45-year experience of the United Kingdom Provident with groups of abstaining and non-abstaining policyholders by no means affords any reliable measure of the alleged

decreased longevity of non-abstainers. And, as the greater presumably includes the less in this case as in all cases, if the larger and longer experience of the United Kingdom Provident Institution, of London, covering forty-five years, does not afford such a measure, of what value are the materially smaller and shorter experiences of the Abstainers and General Insurance Company, of Birmingham, the Sceptre Life Association, Limited, of London, the Scottish Temperance Life Assurance Company, Limited, of Glasgow, the Manufacturers' Life Insurance Company, of Toronto, and the Australasian Temperance and General Life Assurance Society, which were also cited by Dr. McMahon in his paper before the Association of Life Insurance Medical Directors? To be sure, the comparative figures for all of these companies show a considerably higher ratio of actual to expected claims in their General Sections than in their Temperance Sections. But, as no two of the comparisons for long stretches of years even approximately concur in fixing the supposed margin of difference, these supplemental data only tend to confirm the doubt as to the real value of any life insurance experience on these lines. In so far as life insurance experience in the United States is concerned, no companies of any importance now maintain separate sections of abstaining and non-abstaining policyholders, or ever have maintained such sections for sufficient time to afford any evidence of the slightest value.

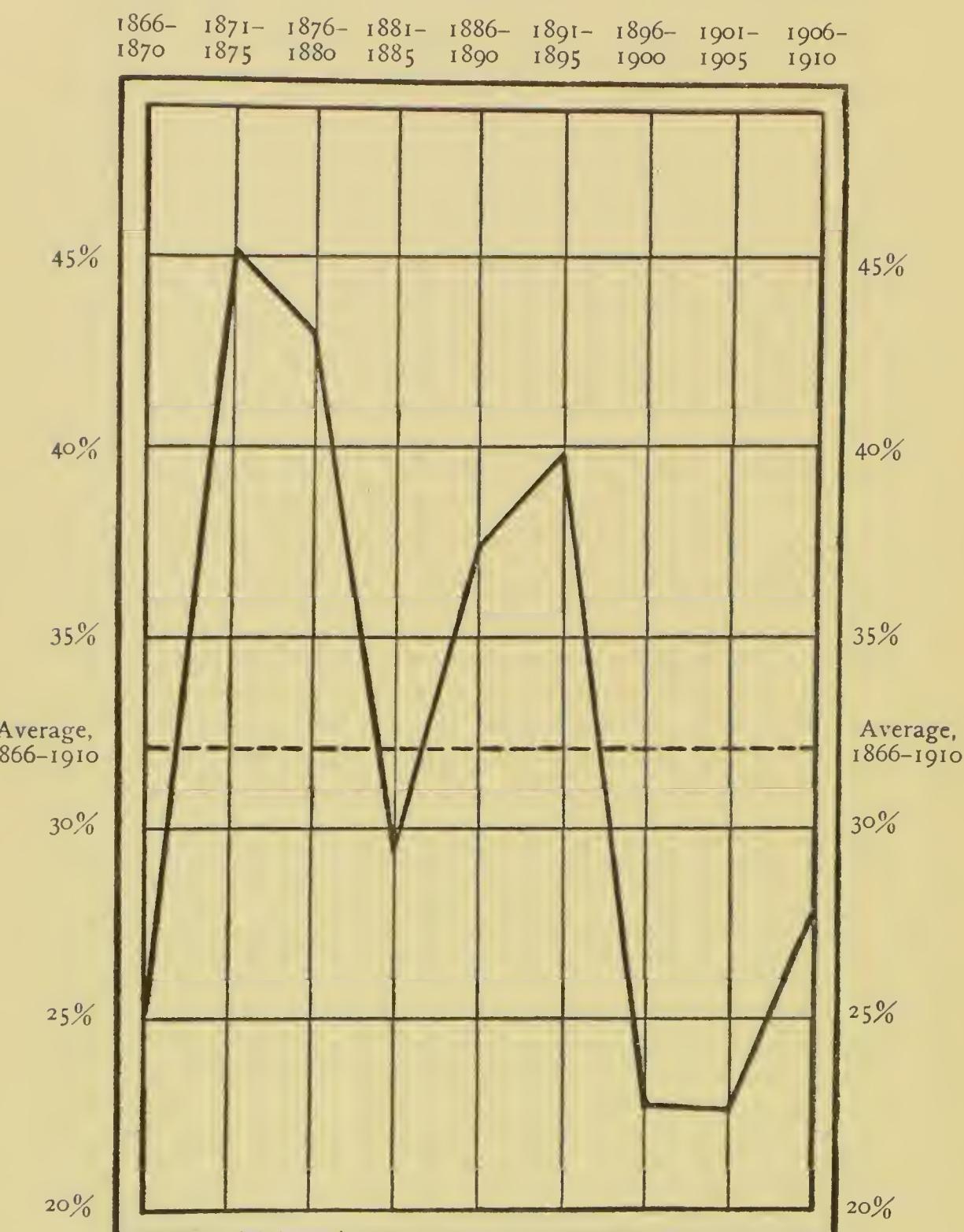
In so far as any life insurance company's mortality experience can be cited by the spokesmen for the temperance cause as alleged mathematical proof and measure of the difference between the death-rates of the two types of men loosely classified as drinkers and non-drinkers, the comparatively broad 45-year experience of the United Kingdom Provident Institution undoubtedly takes precedence of all other supposed evidence on these lines. But, looking at this supposed "evidence" from a purely statistical viewpoint, for the reasons already enumerated I cannot regard the experience in question as of any value as a measure of the presumptive difference between the death-rates of the two groups of men in the world at large. At most, as I see it, it merely corroborates in a general way the common belief that there is a difference between the death-rates of these two types of men, as there doubtless is between any two large groups

of men of widely different habits, amusements, and activities. But the margin of difference between the recorded death-rates of the abstaining and non-abstaining groups of policyholders in the oldest and largest of the life companies maintaining such classes so sharply varies, even when carefully computed by five-year periods, it would seem impossible to believe that the two groups are sufficiently well-defined to be fairly comparable. If the good and bad qualities of the two artificially-separated groups of life insurance risks, or the physiological credits and debits, so to speak, were reasonably constant factors, might it not naturally be expected that the difference between the mortality of the two groups—however large or small it might be—would also be fairly constant?

That the difference by five-year periods is anything but constant I have already shown by the tabulations of the mortality experience of the Temperance Section and General Section of the company in question. To the eye untrained in reading and interpreting figures, however, mere tabulations are generally more or less dumb mysteries, and by way of simplifying in graphic form which any man can instantly read the widely fluctuating differences between the mortality experiences of the two groups in question, I have translated the tabulations in question into the chart which accompanies this paper. In this plainspoken chart the excess of the five-year ratios of actual to expected mortality in the General Section over the corresponding ratios of the Temperance Section is traced by the decidedly-wavy plotted line which records the ever-varying percentage of that excess, and the heavy plotted horizontal line showing the *average* excess for forty-five years by its marked contrast with the other, up-hill-and-down-dale, plotted line sharply emphasizes the fact that the excess was far from being a fixed quantity. It would seem that this chart could scarcely fail to raise a serious question as to the real value of the United Kingdom Provident mortality comparisons in the mind of any impartial student of the alcoholic problem who may happen to glance at it.

But I gravely doubt if the graphic showings of the chart will have any significance in the eyes of the over-zealous type of Prohibition advocates who apparently have no patience with

18 DEATH-RATES OF ABSTAINERS AND NON-ABSTAINERS



A GRAPHIC RECORD OF THE FIVE-YEAR FLUCTUATIONS IN THE DIFFERENCE BETWEEN THE RATIOS OF ACTUAL TO EXPECTED DEATHS IN THE GENERAL AND TEMPERANCE SECTIONS OF THE UNITED KINGDOM PROVIDENT INSTITUTION

any evidence which does not tend to support their contentions. So deeply immersed are they in the movement in which they have enlisted that they are scarcely qualified for jury-duty in cases in which any phase of the alcoholic problem is involved. In his notable address before the Insurance and Actuarial Society of Glasgow, on February 1, 1909, under the title of "Some Observations on the Comparative Death-Rates of Abstainers and Non-Abstainers in Life Assurance Companies," Dr. Ebenezer Duncan, F. F. P. S. G., J. P., remarked: "I have found some strong teetotallers genial, generous, and lovable men, but with characteristic failings, and one of the worst of them is a habit of exaggeration and intemperance in their statements about moderate and temperate indulgence in beverages containing a percentage of alcohol and about the evils, often imaginary, of such moderate indulgence. I would venture to suggest to them that they should give up using the word Temperance in connection with societies which demand, in conformity with their fundamental principles, a pledge for total abstinence from every member, and which also demand that the State shall carry out a policy of total abolition of the sale of alcoholic beverages. The battle of the true temperance party can only be won by rational means directed towards the uplifting of the whole moral and social life of the people—by evolutionary and not by revolutionary measures."

SCHOOL-CHILDREN CALLED ON TO SOLVE A WORLD PROBLEM!

An excellent illustration of this *intemperate* policy of *temperance* advocates to which Dr. Duncan alluded is furnished by one of the latest of the many Temperance leaflets which have come my way since the publication of my book on "The Mortality of Alcohol." The 32-page booklet in question bears the title of "The Effect of Alcoholic Drinks upon the Human Mind and Body," was prepared by the Scientific Temperance Federation, of Boston, was published by the Anti-Saloon League of Maryland, and, as its title-page announces, was intended "for the use of the Public School Pupils of Baltimore in competing for the 400 prizes offered for the best essays on the above subject." Under the heading of "The Prize Offer" the details of the proposed competition are recited at length, and in part read as follows:

"Three gentlemen especially interested in the scientific and health aspect of the temperance question offered \$1,000 each through the Anti-Saloon League of Maryland for a fund for prizes for the best essays written by pupils in the Baltimore Public Schools on the subject: 'The Effect of Alcoholic Drinks upon the Human Mind and Body.' The offer is as follows: The Anti-Saloon League of Maryland hereby offers a cash prize of \$3.00 for the best essay on the above subject written by a pupil in each of the five highest grades in each of the approximately 100 elementary schools of Baltimore City. The League further offers four district prizes of \$10 each for each of these five highest grades, and a city-wide prize of \$50 for each of the five highest grades, making five \$50 prizes, twenty \$10 prizes and from 350 to 360 \$3.00 prizes for the elementary schools. The League also offers a prize of \$10 for the best essay written by a pupil in each of the four regular annual classes in each of the five secondary schools, and a city-wide prize of \$50 for the best essay from each of the four yearly classes in the secondary school system, making twenty prizes of \$10 each and four prizes of \$50 each for the secondary schools."

The pamphlet further states that agreement was made by the League with the Scientific Temperance Federation, of Boston, to prepare and furnish "a 32-page pamphlet devoted entirely to the scientific and health aspect of the question and containing no mention of the Anti-Saloon League or any other temperance organization, of local option or Prohibition, of the saloon as an institution or any other political or controversial phase of the subject," copies of this pamphlet to be furnished free to the 30,000 pupils in the grades open to the prize contest. The pamphlet so prepared, it is announced, was approved by a sub-committee of the Board of School Commissioners of Baltimore, but by a vote of 5 to 4 the Board refused to accept it, and "the Anti-Saloon League, therefore, because of the great public interest manifested, has determined to place a copy of this pamphlet in every one of the more than 100,000 homes in Baltimore City."

The utter lack of scientific promise in this offer of \$3,000 for a prize-competition of 30,000 *school children* with essays on "The Effect of Alcoholic Drinks upon the Human Mind and

Body" is so self-evident that any analysis of its elements of absurdity would be needless. There are comparatively few men in the world who are competent to handle in illuminative and judicial fashion so profoundly complicated a subject; and of course for children scarcely in their teens the subject prescribed for the essays would be a very bottomless abyss. All competent educators are agreed on not only the uselessness but undesirability of calling on children to write on subjects obviously without their mental reach, and the propriety of holding before the eyes of public school children the glittering inducement of considerable cash prizes for concentration of their youthful energies on one detail of physiology, or any other one phase of their school work, would seem extremely dubious. These, however, are matters within the educators' domain, upon which my judgment is merely that of a layman, but I venture to believe my study of the subject has qualified me to some extent to pass upon certain phases of "the effect of alcoholic drinks upon the human mind and body." And I must emphatically dissent from my Prohibitionist friends as to the scientific value of many of the alleged facts and figures cited by them in their anti-alcoholic literature in general, and in the pamphlet intended for the school children of Baltimore in particular.

Practically all of the Prohibitionist literature which I have examined strongly savors of assumptions and exaggeration from the first page to the last, mere estimates and personal opinions as to the effects of alcohol almost invariably being brought forward in the guise of demonstrated facts. One of the pre-requisites for scientific research work is a long-continued series of experiments with each tentative formula, and no investigator with the slightest respect for his reputation would be prepared to announce his conclusions as established facts until he had gone through that *sine qua non* of scientific processes. As to the bad effects upon the human mind and body of excessive use of alcohol, coffee, tobacco, or practically any beverage, food, or other indulgence, of course there can be no question. On this point non-abstainers from alcohol will fully agree with the advocates of total abstinence from alcohol. But a very large percentage of the population of the civilized world vigorously dissents from the Prohibition contention that *all* indulgence in

alcohol, however moderate, necessarily produces bad effects on *all* men, whatever their ages, habits, and manner of living, and declines to accept as proof of that alleged fact the personal opinions and hypothetical estimates of scientific or unscientific investigators who are strongly prejudiced against the use of alcohol. Up to date the scientific investigation of the effects of moderate use of alcohol is in its very infancy, in so far as the deduction of proven and undebatable conclusions is concerned, and as to many phases of the complex subject there are the widest variations of more or less expert opinion. Nevertheless, Prohibition literature almost invariably sets up as positive facts of universal application the assertions and estimates of its sympathizers and spokesmen, and the pamphlet on "The Effect of Alcoholic Drinks upon the Human Mind and Body," which was prepared for the use of the public school children of Baltimore in their proposed prize-essay competition on that subject is no exception.

As to the fairness or unfairness, accuracy or inaccuracy, of that section of the pamphlet which purports to deal with the effect of alcohol upon the human mind, I have no opinion to express. That phase of the subject is one on which only specialists in physiology and psychology can intelligently pass. But with the matter presented under the sub-title of "Drink's Cost in Lives" I am very much interested, especially as at the very outset of that chapter the somewhat startling assertion of "one death every eight minutes due to drink" is made to rest upon the figures presented in my book on "The Mortality of Alcohol," and the specific statement is made that "alcohol carries off 1,662 adults every nine days all the year round in the United States, 65,897 a year, according to the estimate of Edward Bunnell Phelps based on the estimates of medical directors of three of the large American life insurance companies" (p. 22).

I trust I may not seem to place myself in an ungracious position if I flatly deny my responsibility for this statement charged to me, and herewith cite from my book literal proof that I made no such estimate. In the final tabulation on page 64 of my book I did present the figure 65,897 as the possible number of "*deaths in which alcohol may have figured as a causative or contributory factor*," but my "Conclusions" began on page 73 with this para-

graph which, it seems to me, is incapable of the slightest misunderstanding as to its real meaning:

"In default of proof positive to the contrary, it would therefore seem entirely safe to assume that the total annual mortality of Continental United States in which alcohol *directly, indirectly, or even remotely, figures* as a causative or contributory factor at last reports did not exceed the 66,000 deaths approximately suggested by this investigation. It should be clearly understood that *this figure by no means signifies that alcohol was the direct cause of 66,000 deaths*, the number in question presumably including all of the deaths in which alcohol played any appreciable contributory part. Consequently the number of deaths thus computed is not properly comparable with the number of deaths accredited to any particular cause in the annual Mortality Statistics of the Registration Area, as in every case those figures deal with deaths immediately due to the cause named."

I do not see how any person, Prohibitionist or anti-Prohibitionist, could possibly mistake this statement for an estimate on my part that "alcohol carries off 1,662 adults every nine days all the year round in the United States, 65,897 a year." In the case of any death, there may be two, three or half a dozen contributory causes, and surely no one of these more or less remote contributory causes—alcohol or anything else—could properly be charged as *the* cause which "carries off" that particular deceased individual, any more than old age could rightfully be cited as *the* cause of death in all cases of death at advanced ages. Apparently the compilers of the Baltimore pamphlet allowed their zeal to lead them to a serious misinterpretation of the figures presented in my book.

INACCURATE DATA PROFFERED AS GUIDE FOR YOUTHFUL ESSAYISTS

That mistake, to apply the most charitable of constructions to it, is but one of various mistakes, or mis-statements of fact, which I have noted in the pamphlet in question, among them, for instance, being the following assertion of alleged facts on page 22: "In 8 years, 1900-1908, smallpox carried off 2,214 men 25-65 years of age in the registration area of the United States. Typhoid carried off 22,211 men. But alcoholism, for which

alcohol was wholly responsible—and the 75 per cent. of liver cirrhosis which may be charged to alcohol, carried off 33,139 men; 10,928 more than typhoid, and more than fifteen times as many as smallpox."

The references to "8 years, 1900-1908" and "men 25-65 years of age" are somewhat ambiguous (1) as to which of the terminal years of the period is included in the "8 years," and (2) as to the grouping of deaths between ages 25-65, as the mortality statistics for the registration area do not furnish figures for mortality by individual years after age 4, and include the deaths at age 65 in those for the age-group 65-69. Presumably, however, the 8-year period considered was that of 1900-1907, inclusive, and the age-period, 25-64, inclusive, and the figures for those years and that age-period presented in the Mortality Statistics of the Census Office for the years 1900-1907 do not tally with the pamphlet's figures, as this tabulation of the actual returns demonstrates:

MALE DEATHS AT AGES 25-64, INCLUSIVE, IN THE REGISTRATION AREA OF THE UNITED STATES IN THE YEARS 1900-1907, INCLUSIVE, AS RECORDED IN THE CENSUS OFFICE MORTALITY STATISTICS FOR THOSE YEARS

Years	Typhoid Fever	Smallpox	Alcoholism	Cirrhosis of Liver
1900	3,427	205	1,506	1,918
1901	3,293	332	1,426	2,117
1902	3,531	727	1,497	2,198
1903	3,527	485	1,704	2,351
1904	3,394	248	1,497	2,515
1905	2,993	125	1,640	2,492
1906	4,162	42	2,150	3,035
1907	4,167	24	2,474	3,321
Totals for 8-years, 1900-07	28,494	2,188	13,894	19,947

As this tabulation shows, "typhoid carried off 28,494 men of ages 25-64, inclusive, in 1900-1907, inclusive, instead of the 22,211 stated in the Baltimore pamphlet, smallpox carried off 2,188 men, instead of 2,214, and if 75 per cent. of the 19,947 male deaths from cirrhosis of the liver at ages 25-64, inclusive,

or 14,960, were to be added to the 13,894 deaths charged to alcoholism, the total from both causes would be 28,854, instead of 33,139 as alleged in the pamphlet. In other words, the actual excess over the deaths from typhoid, of male deaths so computed from the returns for alcoholism and cirrhosis of the liver at ages 25-64, inclusive, was 360, and not 10,928 as alleged, and there were only thirteen, instead of fifteen, times as many male deaths from these causes as from smallpox at these ages. These mis-statements, or inaccuracies, in the pamphlet's supposedly official figures are of not great consequence, however, except as tending to indicate the general unreliability of specific Prohibition contentions, as the very basis of any such comparisons is so manifestly unfair and misleading.

On the one side, that of deaths due to typhoid and smallpox, only the deaths primarily due to these diseases and so directly charged are included in the comparison; on the other side, not only the deaths directly due to alcoholism according to the official death returns but the exceedingly liberal addition of "the 75 per cent. of liver cirrhosis which may be charged to alcohol" are included. What is the authority for this assumption that 75 per cent. of all male deaths from cirrhosis between ages 25-64, inclusive, may properly be charged to alcohol? None is given, and there is no such authority in so far as I am aware. In the estimates of male mortality at ages 20-74, inclusive, in which alcohol might possibly figure as a causative or contributory factor furnished for my book by the medical directors of three insurance companies, one fixed the possible responsibility of alcohol for deaths apparently due to cirrhosis at 30 per cent. and another one at 90 per cent. This wide margin of difference in expert estimates proves how far apart are trained medical observers on this phase of the alcoholic subject, and it was decidedly presumptuous for the Baltimore pamphlet to announce, without citing any authority, that "75 per cent. of liver cirrhosis may be charged to alcohol." But, as I have said, these details of inaccuracy are of only minor consequence, the attempted comparison of entirely dissimilar items voiding the apparent showings of any value.

The alleged figures for adult male deaths due to typhoid, smallpox, and alcoholism and cirrhosis being incorrect, of course

the diagram in which the "comparative mortality from typhoid fever, small-pox and alcoholism" is supposedly shown, in highly worked-up fashion, is similarly misleading. And, for the reasons discussed at length on the early pages of this paper, the diagram purporting to picture "moderate drinking and the death rate facts from the life insurance companies," on the strength of the United Kingdom and Sceptre Life companies' mortality figures for total abstainers and non-abstainers, seems to me to be distinctly misleading. The last-named diagram does at least have the justification of resting on presumably correctly-quoted figures of the little British life insurance companies in question for their respective "temperance section" and "general section" groups of policyholders, but until much broader and much more carefully classified experience is forthcoming, no diagrams laid out on these lines can be accepted as correctly presenting the relative death-rates of *moderate* drinkers and total abstainers. Undoubtedly *some* of the men who were moderate drinkers when they took out their policies in the British companies in question become immoderate drinkers before they die, and *some* of the men who were not total abstainers when they insured practically if not actually become non-drinkers in later life. How, then, can diagrams worked out on such uncertain and constantly-changing experience be supposed to record accurately the death-rate of *moderate* drinkers, when some of the men so classified probably drink to excess and others so classified practically do not drink at all?

INACCURACY A COMMON WEAKNESS OF PROHIBITION LITERATURE

These, and many other, veins of inaccurate showings and conclusions seem to underlie the entire mass of Prohibition literature, and are by no means wanting in many of the public school text-books on physiology, in the sections devoted to the effects of alcohol and narcotics. According to the World Almanac, "all the States in the Republic have laws requiring the study of scientific temperance in the public schools (whatever that may mean), and all these laws were secured by the W. C. T. U." (the Women's Christian Temperance Union). These laws emanating from a single source, it is but natural that they should be substantially alike in their respective require-

ments and phraseology. Probably for similar reasons, there is considerable uniformity in the treatment of alcohol and tobacco in the physiological text-books used in the public schools in compliance with these laws, and it is extremely probable that any text-book which did not meet with W. C. T. U. approval on these lines would have a rocky road to travel in finding its way into American public schools. The fact that a considerable majority of the male parents of the children in the public schools of this country themselves probably use either alcohol or tobacco to some extent clearly indicates that the introduction of the anti-alcohol and anti-tobacco instruction in the public school curriculum was not made in response to a general public demand, but largely if not entirely through the determined efforts of the W. C. T. U.

The public at large, and certainly all intelligent men irrespective of their Prohibition or anti-Prohibition leanings, undoubtedly would approve of *rational* temperance teaching for their children, and the advisability of such teaching probably would be approved by a substantially unanimous vote if put to the test of the ballot. But there are many careful observers, and by no means necessarily drinking men, who firmly believe that the W. C. T. U. movement has decidedly overshot its mark and worked real injury, instead of good, to the general welfare, not only, for instance, by the misguided abolition of "the canteen" at U. S. Army posts, but in its arbitrary and unreasoning interference with the public schools in the way of insisting on the adoption of biased and unscientific text-books, forcing the study by children of subjects which they cannot grasp, and insisting that the teachers of these children, whatever their own views may be, shall accept and inculcate in their pupils as absolute truths mere opinions on the subject of alcohol and tobacco which have been stamped with W. C. T. U. approval.

Until I became interested in the subject, and took occasion to run over the laws of the various States dealing with these forms of physiological teaching in the public schools, I had no idea of the extraordinary requirements of some of these statutes, or, for instance, that at least 20 States require that their teachers shall pass examinations as to their proficiency in physiology and hygiene. As a rule, properly enough, the laws demand that

physiology and hygiene shall be taught in all public schools, or in all schools in part or in whole supported by public money. Some States exempt certain schools, or grades, from these laws, apparently on the sensible theory that the youngest of school children could scarcely digest instruction in physiology and hygiene, but New York, New Jersey, and several less important States specifically require oral teaching on these lines *for children as yet unable to read!* The use of adequate text-books on these subjects is generally required or implied in the statutes, and State or local boards or superintendents commonly have the say as to which text-books shall be used, Indiana alone exempting its teachers from using text-books unless they so elect.

In some States, principals or teachers must file sworn statements that the laws on these lines have been complied with, and New Hampshire has a statute requiring the superintendent of schools to investigate the teachers' instruction in physiology, with special reference to that regarding alcoholic stimulants. In Pennsylvania, New Jersey, Oregon, South Dakota and Wyoming failure to comply with the law is made sufficient cause for withholding school appropriations; New York, Idaho and North Dakota provide that a teacher's certificate may be revoked for failure or refusal to teach the prescribed course in physiology; and in Ohio the teacher so offending may be fined \$25. Pennsylvania even goes so far as to provide that no person in the habit of using any intoxicating drink as a beverage shall be eligible to teach physiology, and in Iowa the advancement of scholars is made dependent on their proficiency in physiology and hygiene.

The pendulum having swung so extremely far in this direction in the earlier days of the W. C. T. U. movement has latterly tended to swing in the other direction, at least in certain States. In many States the subjects of physiology and hygiene are not now taught in all public schools, and other States have adopted really scientific physiological text-books from which the unscientific and exaggerated statements of some of the earlier text-books have been eliminated, and temperate statements as to stimulants and narcotics substituted. Connecticut set a new high-water mark among States by prescribing an official physiological text-book of its own construction, but it was not radical enough for the Prohibitionists, and they had the law of their

own creation repealed. If I am correctly informed, however, the nominally-discarded official text-book is still being extensively used in the Nutmeg State.

In one of the very latest—and in many respects, the very best—of the text-books of Human Physiology, a copy of which I happen to have, more than twenty of the 362 pages, and many diagrams, are given up to the subject of alcohol and tobacco, and conspicuous in the chapter on “The Effects of Alcohol on the Human Body” is the citation of the mortality experience of the two allegedly distinct sections of the United Kingdom Provident Institution as proof of the relations of “alcohol and length of life.” This citation is followed with this positive, but none the less dubious, statement: “These averages have been made up from records including many thousands of lives, and there is no doubt of their correctness. They have been examined with great care to see if there was any reason other than the use of alcohol why the drinking-man should die earlier than the non-drinker. No such reason can be found, and it is certain that the users of alcohol fail to live as long as those who do not use alcohol, because the alcohol weakens and injures the body.”

Having already discussed at some length the real value of these same mortality statistics, a restatement of my reasons for dissenting from this text-book’s conclusions would here be entirely unnecessary. I should, however, like to know precisely what the author of the text-book means by his phrase “the drinking man,” whether he believes that all drinking-men of any given age have a common death-rate, however much or little they individually drink, and if his flat-footed assertion that “it is certain users of alcohol fail to live as long as those who do not use alcohol” applies to the “rarely use” variety of the New England Mutual Life’s policyholders which was defined by the Company’s Medical Director as “the man who says that he perhaps twice a year at a dinner drinks two glasses of champagne.”

Here is confirmatory, conclusive, and thoroughly up-to-date evidence of the truth of Dr. Duncan’s statement in his paper before the Insurance and Actuarial Society of Glasgow which I have previously cited, to the effect that “one of the worst of them (the characteristic failings of teetotallers) is a habit of

exaggeration and intemperance in their statements about moderate and temperate indulgence in beverages containing a percentage of alcohol and about the evils, often imaginary, of such moderate indulgence." In my judgment, this exaggeration and intemperance in the temperance teachings, and the methods, of the total abstainers and Prohibitionists are largely responsible for the obvious fact that Prohibition has utterly failed to decrease not only in this country, but apparently throughout the civilized world, the *per capita* consumption of alcohol.



